ADDRESS

Delivered by the President, Professor G. H. Darwin, on presenting the Gold Medal of the Society to M. H. Poincaré.

THE medal of the Royal Astronomical Society is this year awarded to M. Henri Poincaré, member of the Academy of Sciences of Paris. As your President, the agreeable duty of presenting the medal to him devolves upon me, but before I do so I must endeavour to lay before you the grounds upon which the Council has made this award.

M. Poincaré's researches have been so diverse in character, and have been carried out with such a wealth of knowledge, that I feel but little confidence in my fitness to perform this arduous task; yet I cannot but rejoice that my tenure of this chair should have furnished me with the opportunity of paying the homage which is due to him for his great achievements in the field of mathematics.

A large part of his work is concerned with the development of the science of pure mathematics, and of this side of his activity I am quite incompetent to speak. But in awarding our medal we naturally think of the value of the contributions by the proposed medallist to our science. Now, although many of M. Poincaré's investigations have perhaps already found, or will at some future time find, their application in the problems of dynamical astronomy, yet it is not necessary to search for cases of possible applicability to astronomy to justify our award. I propose, then, to draw your attention to only three of his lines of research, and these have a directly astronomical scope. My choice is governed not only by the intrinsic interest of the results, but also by the fact that the subjects treated possess a special interest for me. I shall speak, then, of his researches on the dynamical theory of the tides, on figures of equilibrium of rotating masses of liquid, and on the theory of the motions of planets and satellites.

The first of these subjects is treated in two memoirs on the equilibrium and movement of the ocean.* The problem is surrounded by conditions of such intricacy that it seemed to the author advisable to consider the several difficulties separately, as a preliminary to the treatment of the question as a whole. He begins, then, by the equilibrium theory of the tides, but he

* *Liouville's Journal*, 1896, pp. 59–102, and pp. 217–62.

proposes to take into account not only the effect of the obstructing continents, but also that of the attractions of the sea on itself. This problem was solved long ago by Bernoulli for the case when there is no land, and some thirty years ago by Lord Kelvin when there is no mutual attraction of the water on itself.* Lord Kelvin's correction to Bernoulli's theory may be taken as meaning that there are on a certain complete meridian four points at which the semi-diurnal tide vanishes; two of these points are on the same side of the Earth in equal northern and southern latitudes, and the others are antipodal to the first two. There are also four other points on another complete meridian where the diurnal tide vanishes; two of them are in one quadrant in complementary latitudes, and the other two are antipodal to the first two. Lastly, there are two parallels of equal northern and southern latitude at which the tides of long period are evanescent. There are, further, four other points of doubled semi-diurnal tide, four of doubled diurnal tide, and two parallels (sometimes, however, imaginary) of doubled tide of long period.

The positions of these points and parallels are dependent on the distribution of land and sea. The numerical quadratures necessary for their determination have been carried out, and it appears that in every case the points or lines of evanescence or of doubling lie very close to the places where the tide in question absolutely vanishes in Bernoulli's theory.† Since it can make very little difference whether a tide of excessively small range is doubled or annulled, it follows that the correction for land is of little importance, at least when the attraction of the water is neglected.

The introduction of the effects of the mutual attraction obviously presents a problem of great difficulty. Although it is probable that the result would possess little practical importance, yet the question is undoubtedly an interesting one from a mathematical point of view. It is at this point that M. Poincaré takes up the problem, and he shows that it is possible, at least theoretically, to determine a series of harmonic functions by which the mutual gravitation of the ocean may be computed. These functions degrade into ordinary spherical harmonics when the land is all submerged. Lord Kelvin had expressed the opinion that mutual attraction would only change the results to an insignificant degree, but M. Poincaré, while not contesting the justice of this view in general, adduces considerations which seem to show that a distribution of land is imaginable, which might perhaps make the correction of material importance. But the verification of this conjecture would necessitate the complete calculation, which would present an absolutely inextricable complexity on account of the capricious forms of our continents.

Although it does not seem likely that these new harmonic

* Thomson and Tait, *Nat. Phil.* § 808.

† G. H. Darwin and H. H. Turner, *Proc. R.S.* 1886, vol. xl. pp. 303–15.

functions will ever be applied to terrestrial oceans, yet they may perhaps furnish a clue to the discussion of the tides of seas of simple forms, such as the quadrant or octant of the whole globe.

The author's next step is the discussion of the small free oscillations of a system about a configuration of equilibrium, and we are here concerned with principles of the widest generality. He proves that the problem of small oscillations resolves itself into the determination of the maxima and minima of the ratio of the kinetic to the potential energy; and the transition is easy from the case of free oscillations to that of forced oscillations under the action of any perturbing forces. This generalisation forms the basis of the author's whole investigation, and he remarks that he found the first suggestion of it in the work of Lord Rayleigh.

Passing from the case of a finite number to that of an infinite number of degrees of freedom, the author then applies his general principle to the oscillations of liquid standing in a vessel of small dimensions. The vessel is then enlarged so that the free surface at rest cannot any longer be treated as plane, but must be considered as a portion of a sphere concentric with the Earth's centre. It appears that the mathematical analysis is the same as that involved in finding the oscillations of a stretched membrane of unequal density and thickness in its different parts. This problem had been previously treated by the author in another memoir.

This brings us to the end of the first paper, and the second begins with the discussion of the process called by Lord Kelvin the ignoration of coordinates. The so-called gyroscopic terms now appear in the equations of motion; in the tidal problem they arise through the fact of the Earth's rotation, and they form a very essential feature of the whole.

The general principles of the motion in such cases are then applied to the consideration of the oscillations of liquid standing in a small rotating vessel. This problem had been treated previously by Lord Kelvin;* and he found, amongst other results, that waves propagated along a rotating canal would behave differently on the right and left banks. In this he saw a probable explanation of the different behaviour of the tides on the south coast of England and on the north coast of France. Well, the tides on the coasts of the two countries do not differ so much as the treatment which the problem presented by them has received from Lord Kelvin here and from M. Poincaré in France. As might be expected, the latter obtains his solution from the maxima and minima of a certain ratio, which corresponds to that between the kinetic and potential energies in the simpler problems treated before.

* *Proc. R.S. Edinburgh*, 1879 March 17, or *Phil. Mag.* 1880 August, pp. 109–116.

The problem of the rotating vessel only differs from that of the tides in all its generality in the linear scale on which it is supposed to take place; and the effects of the Earth's curvature may be introduced in the same way as in the case of the non-rotating vessel.

The object of these memoirs was not to attain a definite solution in any ideal concrete case, but was to show how the fundamental difficulties might be surmounted by mathematical analysis. Here, as elsewhere, M. Poincaré carries us far beyond the particular instance in view, and it may well be that the principles enounced will meet their actual application sooner in other fields than in the tidal problem.

Important as is the work of which I have just spoken, the memoir on the figures of equilibrium of rotating liquid * seems to me to stand on a much higher level, for it marks an epoch not only in the study of the subject itself, but also in that of many others. It may be that some of the generalisations to be found in it were floating more or less distinctly in the minds of his predecessors, but the theory of the stability of systems in equilibrium or in steady motion has undoubtedly been crystallised and rendered transparent by his efforts. So fundamental are the new conceptions introduced that a new phraseology has become necessary, and it has already been adopted in other investigations, which possess no superficial resemblance to that of which I am now speaking.

Let us imagine a number of mechanical systems which resemble one another in all respects save one, such as size or density or any other measurable quantity. Suppose, further, that the systems are all in equilibrium, and that they are arranged in the order of the magnitude of that one measurable quantity, then we may describe the whole array as a family of systems in equilibrium. Now mechanical systems are often susceptible of equilibrium in more than one configuration, and we may therefore conceive the existence of a second family, which differs from the first in that the equilibrium involves a different configuration of the parts. If we were to examine the two families, we should find that in each the arrangement of the parts changed as we passed along the series. Now it is possible that there would occur in one family a certain member which would resemble the corresponding member of the other family in all respects. If this were the case, this particular member of either family would be described as a form of bifurcation, because it would belong to both, and the two families might be regarded as branching out from it. Now, M. Poincaré proves that if we follow each family towards the form of bifurcation, the equilibrium in one of the families would be stable, while that in the other would be unstable; the same would also hold good after the passage through the form of bifurcation, but the family which was stable

* *Acta Mathematica*, vii. 1885-6, pp. 259-380.

before would be unstable afterwards, and *vice versâ.* There is accordingly exchange of stabilities between the two families. Moreover, the passage of a system from stable to unstable equilibrium necessarily implies that at the moment of change we are passing through a form of bifurcation. Hence the study of the stability of a system may afford notice of the existence of hitherto unsuspected arrangements in which equilibrium is possible. These ideas possess the widest generality, but this is not the place to discuss their fertile ramifications.

If a system in steady motion or one at rest be slightly disturbed, it is said to be stable if the oscillations continue to be small throughout all time. But the existence of even the smallest amount of frictional resistance may betray the fact that stability really means two very different things. In one case even infinitesimal frictional resistance may cause the oscillations to increase to such an extent as to completely change the whole configuration; in another the oscillations may gradually die out and leave the system in the same condition as that in which it was at first. Both systems are stable when there is no friction, but the latter is said to possess secular stability. In nature only systems possessing secular stability are permanent, although it may require a very long time for a system possessing ordinary stability to break down. M. Poincaré here pays a well-merited tribute to Lord Kelvin, who appears to have been the first to clearly indicate this important distinction between these two kinds of stability.

Thus far the discussion is applicable to mechanical systems of every kind, but it finds its special application in the determination of the figures of rotating fluid. It may be well to mention, by the way, that in the course of the paper M. Poincaré justifies a number of statements as to possible forms of equilibrium and their stability, which had been made by Lord Kelvin without proof.

We now come to the main object of the investigation. A planet formed of homogeneous fluid has the form of an oblate spheroid, and its equilibrium is stable. If its angular velocity of rotation be increased, its ellipticity will increase also, but the stability will diminish. When the ellipticity has increased to a certain definite extent, the stability ceases, and for more rapid rotation the figure becomes unstable. At the critical moment of change we are passing through a form of bifurcation, and we know that there must be another series of figures which also possesses that form. This other series consists of the ellipsoids of Jacobi, which have their three axes of unequal magnitudes. But there is one single member of Jacobi's series which is a figure of revolution, and this is identical with the form of bifurcation found by following the stability of the oblate figures. It is true that this Jacobian is also a limiting form, since the series ends there; but we need not stop to consider that point further. It follows from the principle of exchange of stabilities that for rotation slower than

the critical value the Jacobian was stable. All this was known before, but M. Poincaré's work has placed it in a new and clearer light.

Having followed the stable series of oblate ellipsoids of revolution up to the junction at the form of bifurcation, M. Poincaré shunts his train on into the stable branch line furnished by the Jacobian ellipsoids. He follows this branch until he finds the Jacobian to become unstable, and announces that there is a new form of bifurcation, and that a new branch line has been reached. And here the line is nearly blocked by mathematical obstructions, and he is only able to proceed just far enough to perceive that the new figure is pear-shaped, with its larger portion more or less spherical, and with an equatorial protuberance which we may liken to the stalk end of the pear.

This apparently abstract result elucidates the evolution of planetary systems in a very interesting way. Let us consider a rotating liquid mass slowly cooling. If the cooling is slow enough internal friction will cause the whole to revolve throughout with the same angular velocity. At first, when the density is small, the figure will be an ellipsoid of revolution, but slightly flattened; and as it cools the flattening will increase until at a certain stage the figure of revolution ceases to be a figure of equilibrium, and the ellipsoid commences to have an equatorial protuberance; it passes, in fact, into one of Jacobi's ellipsoids. The ellipsoid then lengthens, until at a certain stage it begins to acquire an unsymmetrical furrow in a plane parallel to the axis of revolution, and it becomes pear-shaped, with the axis of revolution at right angles to the core of the pear. "The larger part of the matter tends to approach the spherical form, whilst the smaller part projects from the ellipsoid at one of the extremities of the longer axis, as though it were trying to detach itself from the larger part of the mass. It is difficult to state with certainty what will happen then if the cooling continues, but one may suppose that the mass will go on deepening its furrow more and more, and then it will at last divide itself into two separate bodies by the throttling of the middle part." It is clear that a process of this kind may have played its part in the evolution of celestial systems, and the speculation seems to meet with confirmation from the forms observed in many nebulæ.

M. Poincaré's paper came as a revelation to me, because, just at the time when it was published, I had attempted to attack the question from the other end, and to trace the coalescence of two detached bodies into a single one—but, alas! I have to admit that my work contained no far-reaching general principles—no light on the stability of the systems I tried to draw—nothing of all that which renders Poincaré's memoir one that will always mark an important epoch in the history not only of evolutionary astronomy, but of the wider fields of general dynamics.

I now come to the third contribution of our medallist to

astronomy, namely the work on celestial mechanics. The first of the three volumes * of which the book consists deals with the general principles of dynamics as applicable to the problem of the three bodies, the second is on the work of previous investigators, and in the third the author analyses the results at which he has arrived by the previous discussions. It is probable that for half a century to come it will be the mine from which humbler investigators will excavate their materials. The range of matter is so vast and the number of new ideas so great, that I find myself in considerable difficulty as to how to speak of this book. It would clearly be impossible within the limits of such an address as this to give even an outline of the methods and conclusions.

Under these circumstances, then, I shall limit myself to only one portion of the whole, and shall speak of that at no great length. The subject to which I refer is that of the so-called periodic orbits; and my choice is natural, since I have been myself a hewer of wood and drawer of water in that field.

It was Mr. G. W. Hill who first drew effective attention to this class of solutions of the problem of the three bodies, when he initiated his new method of treating the lunar theory.† I am proud to say that our Fellow, Mr. Ernest Brown, is now carrying on Hill's grand conception to its laborious end. Former mathematicians have, almost without exception, adopted the Moon's elliptic orbit as their starting-point. But the first and roughest approximation to the Moon's path is a circle round the Earth, and the ellipse is an attempt to improve on the circle. Now Hill saw that the early introduction of the ellipse brought in its train a whole series of difficulties, which would be avoided if, continuing to neglect the eccentricity, he improved on the circle by introducing at once the effects of solar perturbation. The distorted circular orbit possesses the great advantage that it always presents the same features towards the Sun and the Earth. It is accordingly possible to draw, with any desired accuracy, a certain definite curve on a plane carried round with the Earth in its orbital motion. This curve exhibits the leading features of the effects of solar perturbation for all time; it resembles an ellipse drawn about the Earth as centre, with the major axis in quadratures, and with the minor axis in syzygies. The principal solar inequality is the variation, and hence Hill describes this orbit as the variational curve, and he uses it as an orbit of reference for the Moon's actual motion. If the Moon had been started with a motion differing but little from actuality she would have followed the variational curve for all time.‡ The curve may then be appropriately termed a periodic orbit.

* *Les Méthodes Nouvelles de la Mécanique Céleste.* Paris, Gauthier-Villars. Vol. i. 1892, vol. ii. 1893, vol. iii. 1899.

† *Researches in the Lunar Theory,* Cambridge, U.S.A. 1877.

‡ Hill's simple variational curve was drawn on the hypothesis that the squares and higher powers of the Sun's parallax are negligible. But these

Hill afterwards introduced the eccentricity of the lunar orbit by reference to the variational curve, and he treated the subject by a fertile method of the highest originality.* He habitually restrains himself from every tendency towards exuberance of expression in his writing, and so I am perhaps deceived when I fancy that even he hardly realised the fundamental importance of what he had done. Speaking of Hill's papers, M. Poincaré writes : "Dans cette œuvre il est permis d'apercevoir le germe de la plupart des progrès que la science a faits depuis." † Although he also pays warm tribute to Gyldèn and to Lindstedt, I conjecture that it may have been Hill who stimulated him to his attack on the profound questions awaiting solution in celestial mechanics. But these are almost personal questions which have little bearing on the advance of science in itself.

The variational curve obviously does not stand isolated, but is merely a single member of a series of periodic solutions of the problem of the three bodies. These solutions, according to M. Poincaré, furnish the only breach by which we may hope to penetrate the fortress of a problem hitherto deemed impregnable. Their importance becomes manifest when I say that he has proved that a periodic solution may always be found which shall differ by as small a quantity as we please from any given motion of the perturbed body, however complicated that motion may be. It is true that the required periodic orbit may need to go through a very large number of circuits before its periodicity is completed.

Orbits of this kind are divisible into three classes : in the first, the inclinations and eccentricities are zero ; in the second, only the inclinations are zero ; and in the third, there are no limitations in these respects. In the cases of the variational curve and of the orbits which I have traced, the orbit of the perturbing body is circular, the whole motion takes place in the plane of the circle, and the periodicity of the perturbed body is completed after a single circuit ; they belong to the first group. As in the investigation of figures of equilibrium, so here also we meet with orbits of bifurcation, limiting forms and exchanges of stability ; and this illustrates the wide generality of the ideas which I attempted to sketch earlier in my address.

Mr. Hill had traced a certain cusped orbit in which the Moon might have moved, and thinking that he had found a limiting form, described it as the Moon of greatest lunation. But M. Poincaré showed that Hill had been misled, and that the cusp is succeeded by looped orbits. Lord Kelvin chose one of them

further approximations may be introduced, and the above statement then becomes correct. The simple curve is symmetrical both as to the lines of syzygies and of quadratures, but the corrected curve is only symmetrical as to syzygies.

* "On Part of the Motion of the Lunar Perigee, &c.," *Acta Mathematica*, vol. viii. pp. 1–36.

† *Méc. Céleste*, vol. i. Introduction.

as an example to illustrate a method of graphical construction by which a curve may be traced by means of its curvature.* The successive transformations of these orbits are elucidated by the series of figures which I have drawn, and I now see that they will terminate their career, at least as simply periodic orbits, in a form which has been called by M. Burrau an orbit of ejection.† As this latter name is hardly sufficiently explanatory in itself, I may say that the perturbed body is supposed to be ejected from one of the two principal bodies along the line of syzygies, or it may fall inward tangentially to that line. These orbits furnish an interesting and peculiar form of periodicity, but the limitation of my time prevents me from referring to them in greater detail.

Another class of orbits, as pointed out by Poincaré, is said to be asymptotic; they are closely akin to the periodic orbits. In this case the body draws asymptotically near to a certain curve, and after performing an indefinite number of circuits, may gradually depart therefrom again. Certain figure-of-eight orbits which I have drawn are intimately connected with these asymptotic orbits.

M. Poincaré has made a searching examination of my figures by means of his analysis, and has put his finger on a weak spot. I had carelessly treated two sets of curves as being continuous with one another, but he shows that my scheme cannot be maintained. I had already arrived at the same conclusion from an entirely different point of view in consequence of the criticism of Mr. S. S. Hough, but it was too late to correct the oversight before the paper was published. Mr. Hough has now a paper in the press for the *Acta Mathematica*, wherein the true sequence of the orbits will be exhibited. I have already by me a large part of the computations required for the actual drawing of a new family of orbits, whose existence I did not suspect.

There can, I think, be little doubt that the investigations of M. Poincaré and of his followers will ultimately afford some sort of explanation of the empirical law of Bode as to the distances of the planets from the Sun; and such an explanation will almost of necessity render intelligible the sequence of processes by which the solar system has been evolved. In the case of the satellites revolving about my ideal planet *Jove* I found that there was a tract near the planet which might be occupied by stable orbits, that this was surrounded by a belt, within which no stable orbit was possible, and that this was again succeeded by a belt of stability. These results are, of course, only true of simply periodic orbits; and I was perhaps rash in saying that I saw in them some indications of a law analogous to that of Bode. Nevertheless, I notice also in M. Poincaré's book a suggestion which may perhaps tend in the same direction. The inferior planets circling round the Sun in my ideal problem were stable up to a certain

* *Phil. Mag.* vol. xxxiv. 1892, pp. 443–8.
† *Ast. Nachr.* Nos. 3230 and 3251.

distance, and then became unstable, and he conjectures that the further pursuit of this family of orbits will lead us back again to a belt of stability. I hope to test this interesting suggestion. But even this meagre sketch has already occupied too much time, and I will say but few words more.

The leading characteristic of M. Poincaré's work appears to me to be the immense wideness of the generalisations, so that the abundance of possible illustrations is sometimes almost bewildering. This power of grasping abstract principles is the mark of the intellect of the true mathematician; but to one accustomed rather to deal with the concrete the difficulty of completely mastering the argument is sometimes great. To the latter class of mind the easier process is the consideration of some simple concrete case, and the subsequent ascent to the more general aspect of the problem. I fancy that M. Poincaré's mind must work in another groove than this, and that he finds it easier to consider first the wider issues, from whence to descend to the more special instances. It is rare to possess this faculty in any high degree, and we cannot wonder that the possessor of it should have compiled a noble heritage for the men of science of future generations.

In handing this medal to you, M. Poincaré, I desire to say on behalf of the Society that in seeking to pay honour to you we feel ourselves honoured.

The meeting then proceeded to the election of Officers and Council for the ensuing year, when the following Fellows were elected :—

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